5-2015

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American Economic Journal: Economic Policy 2015, 7(2): 125–161
http://dx.doi.org/10.1257/pol.20120337

Behind the GATE Experiment: Evidence on Effects of and Rationales for Subsidized Entrepreneurship Training[†]

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Theories of market failures and targeting motivate the promotion of entrepreneurship training programs and generate testable predictions regarding heterogeneous treatment effects from such programs. Using a large randomized evaluation in the United States, we find no strong or lasting effects on those most likely to face credit or human capital constraints, or labor market discrimination. We do find a short-run effect on business ownership for those unemployed at baseline, but this dissipates at longer horizons. Treatment effects on the full sample are also short-term and limited in scope: we do not find effects on business sales, earnings, or employees. (*JEL* I26, J24, J68, L25, L26, M13)

Governments and donors spend billions of dollars subsidizing entrepreneurship training programs around the world. In the United States alone, there exist more than 1,000 Small Business Administration-subsidized Small Business Development Centers (SBDC) and at least 800 other nonprofit programs providing self-employment training and other assistance.[1] Arguments for subsidizing training are manifold, and span theories of allocative and/or redistributive frictions in credit, labor, insurance, and human capital markets. But these arguments have been difficult to evaluate empirically due to classic endogeneity problems from selection into training. Thus, surprisingly little is known about the overall effectiveness of

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We thank David Card, Jesse Cunha, Herb Scheutze, Morten Sorensen, David Sraer, Mirjam van Praag, Martin Sweeney, and seminar participants at the SOLE Meetings, NBER, University of California Berkeley, University of British Columbia, University of California-Santa Cruz, Harvard/IZA Evaluation Conference, Duke/Kauffman Entrepreneurship Conference, HEC Paris Entrepreneurship Conference, Tinbergen Institute, Copenhagen Business School, and WEA meetings for helpful comments and suggestions. Special thanks go to Jonathan Simonetta at the US Department of Labor for responding to our numerous requests for obtaining the experimental data and for ultimately providing the data.

[†]Go to http://dx.doi.org/10.1257/pol.20120337 to visit the article page for additional materials and author disclosure statement(s) or to comment in the online discussion forum.

[1]SBDCs exist in all 50 states and are administered and funded through partnerships between the SBA and public colleges and nonprofits. See http://www.sba.gov/content/small-business-development-centers-sbdcs for a directory of SBDCs, Edgcomb and Girardo (2012) for information on other nonprofit programs, and European Commission (2010) for a description of programs in the European Union.

entrepreneurship training or whether this training mitigates market or redistributive frictions.[2]

We address these limitations by analyzing a large randomized trial on entrepreneurship training in the United States: Project Growing America through Entrepreneurship (GATE).[3] The uniquely large size of this study permits several tests of heterogeneous treatment effects that speak to the key arguments for subsidizing training.

Project GATE was a longitudinal study conducted by the US Department of Labor and the Small Business Administration (SBA) in which free entrepreneurship training was randomly offered to individuals interested in starting or improving a business. More than 4,000 individuals applied for a limited number of slots at 14 different SBDCs and nonprofit community-based organizations (CBOs) located across seven sites in three states. SBDCs and CBOs are the predominant providers of entrepreneurship training services in the US market. Subjects assigned to the treatment group were offered an array of best-practice training services, whereas subjects assigned to the control group were not offered any free services. Follow-up surveys at 6, 18, and 60 months after treatment assignment yield a rich set of outcome measures. The 60-month follow-up provides rare measures of long-run outcomes.

Our estimates of average treatment effects suggest that entrepreneurship training has limited impacts on business ownership, scale, and income. Entrepreneurship training does dramatically increase the likelihood of business ownership in the short-run (by 13 percentage points in the full sample at the 6-month follow-up, on a base of 36 percentage points), but this effect depreciates over time: we do not find significant effects at 18 or 60 months. Nor do we find evidence that training affects other outcomes—including measures of business scale, business profitability performance, household income, and work satisfaction—at any horizon (6, 18, or 60 months). We show that these estimates are not overly sensitive to reasonable assumptions about how attrition affects the composition of the treatment and control groups. Overall, the only significant full-sample average treatment effects, across a large number of tests, are on six-month business ownership and six-month employment status.[4]

[2] In contrast, a large literature evaluates job training and job search assistance programs (e.g., see Card, Kluve, and Weber 2010).

[3] The only previous randomized trial conducted in the United States was a smaller demonstration experiment of self-employment training for Unemployment Insurence (UI) recipients in Washington and Massachusetts (Benus et al. 1994). That study found positive program impacts on self-employment, total earnings, and job creation, but in addition to training the assistance program allowed for concurrent UI benefit payments and a lump-sum benefit payment. Several recent experiments of the effects of business training on micro-entrepreneurs have been conducted in developing countries (Berge, Bjorvatn, and Tungodden 2011; Drexler, Fischer, and Schoar 2014; Karlan and Valdivia 2011; Karlan, Knight, and Udry 2012; Field, Jayachandran, and Pande 2010). These studies have generally found some positive, but mixed, results. The results of this literature may be informative, but not generalizable, to the developed country context, in which the content of entrepreneurship training, education level of trainees, and types of businesses being created are very different, and where formal labor, financial and business markets are more open and accessible. For related research using nonrandomized approaches to identifying effects of self-employment training programs, see, e.g., Kosanovich and Fleck (2001), Rodriguez-Planas (2010), Almeida and Galasso (2010), and for random and quasi-experimental approaches to studying entrepreneurship education for college and younger students, see Huber, Sloof, and van Praag (2012) and Oosterbeek, van Praag, and Ijsselstein (2010).

[4] Our estimates of full-sample treatment effects differ from those in the final evaluation report delivered to the US Department of Labor (Benus et al. 2009) due to several differences in approaches to analyzing the data, including dealing with attrition and nonresponse. Section IVG provides details.

The lack of significant average treatment effects does not appear to be due to a weak treatment (lack of compliance, quality, intensity, a sunk cost effect,[5] etc.). We find that the GATE assignment to treatment produced a 136 percent short-term increase and a 45 percent long-term increase in the amount of training received. Recipients reported the training as useful in follow-up surveys, and the treatment group was 11–13 percentage points more likely to create a business plan. As discussed above, we do find very large short-term effects on business ownership. In all, the results strongly suggest that training changes short-term behavior but not long-run outcomes.

We also provide novel results on heterogeneous treatment effects, using these interactions to shed light on the empirical importance of various rationales offered for training subsidies. Credit constraints are one rationale offered for training subsidies: if training is valuable but potential recipients lack the liquidity to pay for it, offering low-cost training may be a cost-effective way to improve access (compared to subsidizing lending, for instance). Training may also improve financial access by providing information, advice, and assistance in obtaining financing. Labor market discrimination is a second rationale for training subsidies: if minorities face greater discrimination from employers than from customers or lenders, then subsidizing training may be a relatively efficient method of helping minorities overcome barriers to starting businesses and avoid future discrimination in the labor market. A third rationale for training subsidies is human and managerial capital constraints: if education or managerial labor markets do not function well, then low-cost training may improve efficiency or efficiently redistribute services to the most-affected parties. Unemployment insurance frictions are a fourth rationale for training subsidies: training may be a relatively efficient way to insure against job loss by providing recipients with incentives to work by creating a job for themselves (and perhaps others).

We do not find evidence supporting the credit constraint, discrimination, and human capital constraint arguments. We do find limited support for the unemployment insurance friction hypothesis: the effect of entrepreneurship training on business ownership at six months is significantly greater for those who were initially unemployed compared with those who were employed at baseline. However, we do not find any other evidence of relatively strong effects for the unemployed, nor do we find any evidence of lasting effects for the unemployed.

In all, the lack of positive treatment effects in the full sample and in key subgroups, is particularly striking, given that any reporting biases in the follow-up surveys probably push in the direction of finding positive effects.[6]

The rest of the paper proceeds as follows. Section I provides more details on GATE, including its research design and implementation, the nature of the training services received by subjects, and external validity. Section II presents first-stage

[5] A sunk cost effect refers to the possibility that those who receive free training would have purchased training had it not been offered for free, but then they took it less seriously because it was free. This would lead to an underestimate of the impact of unsubsidized training.

[6] It strikes us as sensible to worry that treated individuals might report better outcomes to self-justify their own investment in training, to express gratitude to the training provider or funder for getting free training, and/or if they are mistakenly concerned that eligibility for continued support is contingent on showing progress.

results on training and business practices. Section III presents estimates of average and distributional effects on business ownership, scale, and performance, including a bounds analysis addressing differential attrition. Section IV presents estimates of heterogeneous treatment effects to test hypotheses about the (redistributive) efficiency of self-employment training. Section V concludes.

I. The Growing America through Entrepreneurship (Project GATE) Experiment

A. *Evaluation Design*

Growing America through Entrepreneurship (Project GATE) was an evaluation designed and implemented by the US Department of Labor (DOL) and the US Small Business Administration (SBA). GATE's objective was to "help emerging entrepreneurs in rural and urban communities achieve the American dream of owning their own business."[7] The evaluation was designed to capture existing representative training providers (Section IIB) and recipients (Section IID). The treatment phase of the evaluation ran from September 2003 to July 2005 in seven sites that represented both urban and rural areas. Follow-up surveys were mailed 6, 18, and 60 months after random assignment.

Individuals entered the study by completing an application process for a standard offer of free training from 1 of 14 established providers described in Section IIB.[8] The application process started with an orientation meeting at 1 of 21 One-Stop Career Centers in the 7 sites. Anyone attending the orientation meeting could then apply by completing and mailing a form with questions on demographics, work and business experience, and the individual's current business or new business idea. Applicants were informed that "GATE does not have space for everyone" and that a "lottery or random drawing will decide whether you will be able to enter the program." This would not necessarily be perceived as unusual, as training providers often face capacity constraints.

Program coordinators randomized applicants to treatment or control with equal probability (we confirm balance on baseline observables in Section IIE). Program administrators for each training provider then offered treatment applicants a standard array of free training services (Section IIB), told control applicants that the GATE program did not have the capacity to offer them services, and did not offer control applicants referrals to any other services. Individuals in both treatment and control groups were notified that they would be mailed follow-up surveys.

GATE is the largest-ever randomized evaluation of entrepreneurship training and assistance, with 4,197 individuals randomized at baseline.

[7] See http://www.doleta.gov/projectgate/ for more information.

[8] Training providers marketed GATE through several channels: online; on-site electronic kiosks, merchandising, and paper materials; direct mail (insert with UI checks); mass media; and referral networks with community-based organizations.

B. *Services Provided and the External Validity of the Training "Treatment"*

GATE training providers were chosen with a goal of (not quantitatively determined) representativeness of the subsidized training market.[9] Fourteen organizations from seven different sites participated in the GATE study, with a mix of SBA-funded Small Business Development Centers (SBDCs) and nonprofit community-based organizations (CBOs) in both urban and rural locations. The 14 participating providers deliver services in and around Philadelphia; Pittsburgh; Minneapolis/St. Paul; Duluth, Minnesota; Virginia, Minnesota; Portland, Maine; Lewiston, Maine; and Bangor, Maine (see Bellotti, McConnell, and Benus 2006 for more details). SBDCs and CBOs offer similar services, as detailed below, but differ somewhat in their stated goals. The SBDCs tend to emphasize helping small business owners grow (or start) their businesses to contribute to the local economy, whereas the CBOs tend to emphasize small business ownership as a path to self-sufficiency. Both types of organizations employ experienced business consultants to deliver one-on-one and group trainings.

Eighty-nine percent of the treatment group actually received some training during the evaluation horizon, with 81 percent getting training within 6 months of entering the study. GATE training was customized for the individual from an array of services offered by the provider, as is typical in the subsidized market.[10] Training began with a one-on-one assessment to produce a service plan that typically combined one-on-one services with selected group services. Sixty-four percent of treatment group individuals then received one-on-one counseling/consulting that was customized to the individual's experience, capabilities, circumstances, and opportunities. Seventy-seven percent of the treatment group received classroom/group training(s). These targeted a variety of general and specialized topics at different experience levels. Introductory workshops covered subjects such as legal structure, business plans, and marketing. Intermediate and advanced group trainings covered subjects including managing growth, obtaining financing, legal risks, and personnel issues. More specialized group trainings covered topics such as accounting, information technology, and web-based businesses. Benus et al. (2009) estimate that the total cost of providing training to GATE recipients is $1,321 per person.

C. *Data and Design Limitations Preclude Unpacking Heterogeneity in Training Content/Delivery*

Study limitations preclude identifying any heterogeneous effects for different types of training content or providers. Training content was not randomized: each member of the treatment group was offered a one-on-one assessment and was

[9]For small businesses at least, casual empiricism suggests that the subsidized market is larger than the nonsubsidized market. For example, on September 3, 2013 the top three Google links for "small business training" were all SBA-related, while the fourth link was to the General Services Administration (the federal government's procurement agency), which also provides subsidized training.

[10]For example, the SBA describes SBDCs as providing "...extensive, one-on-one, long-term professional business advising, low-cost training and other specialized services" (http://www.sba.gov/content/small-business-development-centers-sbdcs).

advised of a menu of services, as described directly above. Training provider identities are suppressed in the data for confidentiality reasons; the only related information we observe is the proportion of study participants in different "sites" (which are aggregated to Philadelphia, Pittsburgh, Minneapolis/St. Paul, Duluth/Virginia, Minnesota, and Maine in the microdata), and whether participating providers in these sites were SBDCs and/or CBOs (Bellotti, McConnell, and Benus 2006).[11]

D. *Study Participant Characteristics and External Validity of the Sample*

GATE was designed to estimate treatment effects on recipients who are representative of those served by subsidized training providers. GATE services, like most subsidized training programs in the United States, were marketed to any individual interested in starting or growing a business.[12]

What do subsidized training recipients look like, typically? We are not aware of any (other) nationally representative data on the characteristics of *training entrants*, but data sources on *self-employment entrants* suggest that we should expect to find high rates of unemployment at our baseline sample, since both voluntary and involuntary unemployment are strongly associated with subsequent entry into self-employment (Farber 1999; Parker 2009; Krashinsky 2005; Fairlie and Krashinsky 2012; Fairlie 2013).[13] It is thus reassuring to find that 55 percent of our sample is unemployed at baseline, with 39 percent receiving unemployment insurance (Table 1).

Returning to the full GATE sample, Table 1 describes several other baseline characteristics (besides employment status) that we use below to test for treatment effects in subsamples motivated by various rationales for subsidizing training. Much of the sample plausibly faces credit constraints, with 44 percent reporting a bad credit history[14] and many belonging to groups thought to be subject to labor market discrimination (46 percent are females, and nearly 50 percent are minorities). A majority of the sample plausibly lacks specific human capital, with 19 percent of participants already self-employed and 32 percent of participants having ever worked for relatives or friends who were self-employed.

In all, the available data suggests that GATE succeeded in obtaining a representative sample of subsidized training recipients, and that various subgroups of particular interest are also well-represented.

[11] We use this information to infer that GATE service delivery in the Pittsburgh and Duluth areas was dominated by SBDC providers, with the Philadelphia area served entirely by CBOs. Treatment effect estimates for these two subsamples are similar to those for the full sample (Appendix Tables 1A and 1B), although the small subsample sizes produce wide confidence intervals that do not rule out big differences across the different areas/provider types.

[12] Some smaller-scale programs target recipients of social insurance. Demonstration programs in Washington and Massachusetts starting in 1989, and Self-Employment Assistance programs in several states starting in 1993, targeted unemployment insurance recipients and provided concurrent UI benefits or lump sum payments (which also exists in Europe, e.g., Baumgartner and Caliendo (2008). The Self-Employment Investment Demonstration, implemented from 1988 to 1992 in five states, targeted AFDC recipients.

[13] Involuntary employment may be a spur to occupation change, and voluntary employment may be an optimal step along a transition path to self-employment if starting a business and/or obtaining training is time-consuming.

[14] A large percentage of the sample might also lack collateralizable wealth given the prevalence of modest incomes (33 percent $<$ \$50,000 annual household income).

TABLE 1—TREATMENT/CONTROL COMPARISON OF CHARACTERISTICS FOR GATE EXPERIMENT

	Baseline			Follow-up wave 1		
	Treatment group (1)	Control group (2)	*p*-value for treat-control (3)	Treatment group (4)	Control group (5)	*p*-value for treat-control (6)
Philadelphia	28.7%	27.5%	0.40	26.8%	25.6%	0.43
Pittsburgh	13.8%	14.6%	0.43	13.7%	14.3%	0.58
Minneapolis-St. Paul	39.8%	39.0%	0.58	41.1%	39.1%	0.24
Duluth	4.6%	5.0%	0.54	4.6%	5.1%	0.51
Maine	13.1%	13.9%	0.48	13.9%	15.9%	0.09
Female	47.2%	45.7%	0.32	48.5%	46.4%	0.22
Black	30.5%	30.6%	0.91	29.1%	29.8%	0.65
Latino	6.2%	5.1%	0.12	6.3%	4.9%	0.09
Asian	4.6%	4.5%	0.86	3.8%	3.3%	0.42
Other	7.9%	8.1%	0.80	7.7%	7.6%	0.91
Not US born	10.0%	10.2%	0.83	8.9%	9.2%	0.81
Age	42.08	42.77	0.03	42.73	43.42	0.04
Married	48.1%	48.4%	0.81	49.4%	48.6%	0.64
Has children	46.7%	46.1%	0.68	45.4%	45.1%	0.88
Highest grade completed	14.39	14.52	0.07	14.53	14.61	0.28
HH Income $25,000–49,999	32.6%	33.7%	0.46	33.0%	34.0%	0.56
HH Income $50,000–74,999	17.9%	17.2%	0.55	18.5%	17.5%	0.45
HH Income $75,000–99,999	6.9%	7.2%	0.70	7.1%	7.2%	0.91
HH Income $100,000+	6.3%	7.0%	0.31	6.9%	7.4%	0.56
Self-emp. at application	18.3%	19.5%	0.33	19.3%	20.4%	0.41
Has a health problem	8.7%	8.3%	0.63	9.0%	8.9%	0.90
Has relatives or friends who have been previously S.E.	70.3%	70.4%	0.93	71.7%	72.0%	0.85
Ever worked for relatives or friends who are S.E.	31.7%	32.0%	0.81	31.7%	31.8%	0.96
Has a bad credit history	45.4%	43.9%	0.34	43.3%	43.2%	0.94
Currently receiving UI benefits	39.9%	38.1%	0.24	41.1%	39.7%	0.40
Has health insurance from current employer	16.8%	18.1%	0.26	16.6%	17.5%	0.48
Autonomy index	1.7%	−1.7%	0.27	−1.1%	−1.9%	0.81
Risk tolerance index	−0.2%	0.2%	0.87	2.6%	−1.1%	0.27
Unemployed at application	55.3%	55.4%	0.92	55.0%	55.5%	0.78
F-test for all variables			0.56			0.53
Sample size	2,094	2,103		1,758	1,691	
Percent of baseline sample	100.0%	100.0%		84.0%	80.4%	0.003

(*Continued*)

E. *Randomization Integrity and Differential Attrition*

Table 1 checks for treatment versus control balance on characteristics at the baseline and at each of the three follow-ups (sample composition changes over time due to attrition). Random assignment was not stratified by site, but the top rows show that each site produced roughly 50–50 assignments nevertheless (columns 1–3). Among the numerous baseline characteristics measured in the application, only age is statistically different between treatment and control. One would expect to find one or two significant differences by chance, and the magnitude of the age difference is small (< 1 year). In any case, when estimating treatment effects we present results both without covariates as well as with controls for a large set of detailed baseline characteristics.

Table 1—Treatment/Control Comparison of Characteristics for GATE Experiment (*Continued*)

	Follow-up wave 2			Follow-up wave 3		
	Treatment group (7)	Control group (8)	*p*-value for treat-control (9)	Treatment group (10)	Control group (11)	*p*-value for treat-control (12)
Philadelphia	25.1%	24.0%	0.49	23.1%	22.0%	0.53
Pittsburgh	14.0%	14.2%	0.82	14.5%	14.4%	0.92
Minneapolis-St. Paul	42.3%	40.4%	0.29	43.9%	42.0%	0.35
Duluth	4.7%	5.1%	0.60	5.0%	4.9%	0.99
Maine	14.0%	16.3%	0.08	13.6%	16.7%	0.03
Female	48.8%	46.9%	0.31	48.1%	47.1%	0.62
Black	27.6%	28.3%	0.69	25.3%	26.0%	0.70
Latino	6.4%	5.1%	0.12	6.4%	5.2%	0.19
Asian	3.3%	2.9%	0.52	3.1%	2.8%	0.71
Other	7.4%	7.0%	0.64	7.4%	6.6%	0.47
Not US born	8.3%	8.7%	0.67	7.1%	8.1%	0.34
Age	43.16	43.81	0.07	43.91	44.16	0.54
Married	50.2%	49.0%	0.54	51.4%	49.6%	0.38
Has children	45.4%	44.6%	0.69	44.0%	42.8%	0.58
Highest grade completed	14.59	14.66	0.38	14.75	14.78	0.77
HH Income $25,000–49,999	32.9%	33.4%	0.77	31.9%	34.5%	0.18
HH Income $50,000–74,999	19.2%	17.8%	0.31	20.1%	17.2%	0.06
HH Income $75,000–99,999	7.4%	7.3%	0.92	8.1%	7.4%	0.53
HH Income $100,000+	7.5%	8.0%	0.59	8.8%	8.9%	0.96
Self-emp. at application	19.8%	21.2%	0.34	20.3%	21.5%	0.48
Has a health problem	9.1%	8.9%	0.85	8.9%	8.4%	0.69
Has relatives or friends who have been previously S.E.	72.9%	72.5%	0.81	73.6%	73.1%	0.78
Ever worked for relatives or friends who are S.E.	31.6%	31.7%	0.97	30.9%	31.5%	0.77
Has a bad credit history	41.8%	41.5%	0.87	38.9%	39.4%	0.79
Currently receiving UI benefits	42.1%	39.3%	0.12	43.0%	41.1%	0.35
Has health insurance from current employer	16.6%	17.6%	0.46	16.8%	17.1%	0.84
Autonomy index	−0.7%	−1.7%	0.79	−2.0%	−4.9%	0.49
Risk tolerance index	1.3%	−2.0%	0.34	−0.7%	−4.4%	0.35
Unemployed at application	55.5%	54.6%	0.63	55.8%	55.4%	0.85
F-test for all variables			0.69			0.80
Sample size	1,563	1,475		1,274	1,176	
Percent of baseline sample	74.6%	70.1%	0.001	60.8%	55.9%	0.001

Notes: All reported characteristics are measured at time of application, prior to random assignment. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. The autonomy index is created from standardizing responses on a scale of 1 to 5 to whether the statement "I enjoy working independently" is true about themself. The risk aversion index is created from combining standardized responses to "I'm only willing to take a risk if I am sure everything will work out" and "I am not prepared to risk my savings for my business."

Table 1 also compares treatment and control completion rates and baseline characteristics for each of the three follow-up surveys. The bottom row shows that control group members are significantly more likely to attrit: the completion rate differs by 4–5 percentage points, on a base of 56–80 percent, for each follow-up wave. However, despite differential attrition rates overall, we do not find differences in the observable composition of the treatment versus control groups, based on characteristics observed in the baseline. The number of significant differences is about what one would expect to find by chance, and the magnitude of these differences is small. More formally, in a regression of follow-up survey completion on baseline characteristics, treatment status, and baseline characteristics interacted with treatment

status, the *F*-tests on the interaction variable coefficients have *p*-values of 0.214 for Wave 1, 0.823 for Wave 2, and 0.091 for Wave 3. Despite this reassurance, we investigate how treatment effects might be biased if there is in fact differential attrition (e.g., on unobservables) in Section IIIF.

F. *Empirical Strategy*

Our main specification for estimating average treatment effects on outcomes focuses on estimating the effects of *receiving* entrepreneurship training (i.e., local average treatment effects) instead of estimating the effects of being *offered* free entrepreneurship training (i.e., "intent-to-treat" effects). We estimate first-stage OLS regressions of the form

$$E_{it} = \omega + \boldsymbol{\gamma}\mathbf{X}_{ib} + \pi T_{ib} + u_{it}. \tag{1}$$

Where E measures whether individual i had obtained any training by follow-up survey t,[15] $\mathbf{X}_{ib}$ is a vector of the baseline covariates (indexed by b for "baseline") reported in Table 1, and $T_{ib} = 1$ if i was assigned to the treatment group. The second-stage regression for an outcome of interest y, measured for individual i at time t, is then

$$y_{it} = \alpha + \boldsymbol{\beta}\mathbf{X}_{ib} + \Delta\hat{E}_{it} + \varepsilon_{it}, \tag{2}$$

where $\hat{E}_{it}$ is the predicted likelihood of training receipt and u_{it} and ε_{it} are error terms. Δ provides an estimate of the local average treatment effect (LATE). When estimating heterogeneous treatment effects we add interactions between baseline covariates and treatment assignment to the equation (2).

II. The First Stage: How Powerful Is the Experimental Treatment?

A. *Effects on Total Training Quantity and Quality*

Given that the control group was not restricted from obtaining training elsewhere, it is important to examine whether and how the GATE treatment actually changed the use of training services. If each member of the control group simply obtains services elsewhere, or obtains better-quality services elsewhere, then the experimental design will not identify the causal effects of (subsidized) training. We consider both quantitative and qualitative effects of GATE's random assignment on the totality of training received by individuals in the study.

Starting with the quantity of training received, Table 2, column 1 shows that the treatment group is an estimated 37 percentage points more likely (81 percent versus 44 percent) to receive any training by Wave 1 (6 months following random

[15] We also estimate first-stage relationships between treatment assignment and intensive margins of training receipt (see Section III). Note that we only have one instrument and hence cannot separately identify effects on extensive and intensive margins.

Table 2—Treatment and Control Groups Receipt of Entrepreneurship Training

	R.A. to wave 1 (6 month period)		Wave 1 to wave 2 (12 month period)		Year prior to wave 3 (12 month period)	
	Percent receiving (1)	Mean hours (2)	Percent receiving (3)	Mean hours (4)	Percent receiving (5)	Mean hours (6)
Treatment group						
Any entrepreneurship training	81.2%	15.6	41.5%	7.3	26.1%	4.6
Attended classes, workshops or seminars	66.8%	13.8	35.0%	6.6	22.1%	4.0
Counseling or technical assistance	52.5%	1.8	18.0%	0.8	10.0%	0.6
Control group						
Any entrepreneurship training	44.0%	6.6	37.9%	6.7	28.7%	5.7
Attended classes, workshops or seminars	37.7%	5.8	32.7%	6.1	25.1%	5.2
Counseling or technical assistance	19.2%	0.9	13.8%	0.7	10.3%	0.6
Treatment-control difference and (*standard error*)						
Any entrepreneurship training	0.372	8.99	0.036	0.63	−0.026	−1.10
	(0.015)	(0.72)	(0.018)	(0.73)	(0.018)	(0.71)
Attended classes, workshops or seminars	0.290	7.97	0.024	0.50	−0.030	−1.15
	(0.016)	(0.68)	(0.017)	(0.68)	(0.017)	(0.68)
Counseling or technical assistance	0.333	0.99	0.043	0.10	−0.003	0.05
	(0.015)	(0.15)	(0.013)	(0.11)	(0.012)	(0.17)

(*Continued*)

assignment) than the control group.[16] The treatment group also received more than twice the number of hours of training by Wave 1: 15.6 versus 6.6 (column 2).[17] Cumulating across waves, the treatment group was 23 percentage points more likely to receive any training (column 9) and received 8.5 more hours (column 10).

Can a difference of 8.5–9 hours of training reasonably be expected to affect business outcomes? We believe the answer is yes, for several reasons. First, the 8.5–9 hour differential only measures training time, not any "homework multiplier" (see, e.g., Table 3 for a big treatment effect on business planning). If we were to evaluate the effectiveness of a standard five-credit college course, for example, we would not think of any treatment effects as working merely through the 30 or so hours of instructional time. Nor would we think of the impact of preventative medical care as working merely through the few hours (or minutes) of office visits; rather, it is the provision of key bits of information, and how recipients then apply that information in their daily lives (and/or businesses), that is important. Second,

[16] Examining who receives entrepreneurship training, we find only a few characteristics that predict take-up of training by each follow-up wave. Focusing on the main effects we find some evidence that African Americans and the more educated are more likely to receive training (see Appendix Table 2). Examining differential take up between the treatment and control groups, we find only a few significant differences. *F*-tests for differential take up for all covariates do not reject equality in any of the three follow-up waves. Unfortunately, no information was gathered at baseline on whether participants ever received training prior to random assignment.

[17] The levels and differences in training receipt are similar across program sites.

Table 2—Treatment and Control Groups Receipt of Entrepreneurship Training (*Continued*)

	Cumulative to wave 2		Cumulative to wave 3	
	Percent receiving (7)	Mean hours (8)	Percent receiving (9)	Mean hours (10)
Treatment group				
Any entrepreneurship training	86.4%	22.9	88.7%	27.6
Attended classes, workshops, or seminars	74.1%	20.4	77.0%	24.4
Received one-on-one counseling or technical assistance	58.8%	2.6	63.7%	3.3
Control group				
Any entrepreneurship training	57.3%	13.3	65.4%	19.1
Attended classes, workshops, or seminars	50.6%	11.9	58.7%	17.1
Received one-on-one counseling or technical assistance	27.9%	1.5	34.4%	2.1
Treatment-control difference and (standard error)				
Any entrepreneurship training	0.291	9.62	0.232	8.52
	(0.016)	(1.15)	(0.017)	(1.53)
Attended classes, workshops or seminars	0.235	8.47	0.183	7.31
	(0.017)	(1.08)	(0.019)	(1.38)
Received one-on-one counseling or technical assistance	0.310	1.09	0.293	1.14
	(0.017)	(0.21)	(0.019)	(0.30)

Notes: The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application.

the limited available prior evidence suggests that just a few hours of training can be impactful (Drexler, Fischer, and Schoar 2014).[18] Third, casual empiricism reinforces the notion that the first few hours of training or advice might be the most impactful; after all, many board members and advisors of small companies only provide a handful of hours of advice or training each quarter.

Returning to Table 2, note also that the cumulative differences in training hours are driven by the Wave 1 effects; this is unsurprising, given that the sample is comprised of people with demand for training at the time of random assignment. We would only expect to find differences in training obtained at later horizons if there is strong complementarity between training obtained in the short-run (between random assignment and Wave 1 follow-up) and training obtained later (between Wave 1 and Wave 2, and/or between Wave 2 and Wave 3).

Can short-run differences in training generate longer-run differences in business outcomes? In other words, is it reasonable to expect that training, if effective, will produce differences in business outcomes at Wave 2 and Wave 3, given that training receipt only differs at Wave 1? Yes, if training is an upfront investment (in human capital) that produces a flow of returns into the future.

Table 2 and Appendix Table 3 also report evidence on how treatment assignment affects the type/quality of training received. The results suggest that an individual in

[18] This evidence is from a different setting (microcredit users in the Dominican Republic), and its external validity to our setting is uncertain.

the treatment group is far more likely to obtain customized training; e.g., 30 percentage points more likely to receive one-on-one assistance (Table 2), and 16 percentage points more likely to receive help refining their business idea (Appendix Table 3). Appendix Table 3 also suggests that treatment group respondents found the training to be high-quality, with 52 percent of recipients reporting it "very useful" and 34 percent "somewhat useful." The comparable proportions for those in the control group who obtained any training are 36 percent and 41 percent. Appendix Table 3 also reports treatment and control group responses to questions about whether training helped with 12 specific aspects of business planning and operation. (The sample is again comprised of training recipients only). The treatment group is more likely to respond that training was helpful for each of the 12 aspects.

In all, the evidence suggests that the experiment produced training in substantially greater quantity and quality for treatment relative to control individuals. Hence, any null effects are likely due to training that is ineffective per se, rather than to a low-powered identification strategy.

B. *Effects on Business Planning and Loan Applications*

Table 3 shows that GATE affected some business planning and practice outcomes as well. Treated individuals were 13 percentage points more likely to have written a business plan by Wave 1, and this difference persists over time. We do not find any differences in loan applications, however, on a low base; e.g., only 6 percent of the treatment and control groups applied for a business loan by Wave 1.[19]

III. The Effects of Entrepreneurship Training on More Ultimate Outcomes

A. *Average Effects on Business Ownership*

Table 4 reports estimates of the IV specification in equation (2).[20] Results with and without controls for baseline covariates are similar, and our discussions below focus on the former (column 2). The average impact of entrepreneurship training on business ownership at Wave 1 (the six-month follow-up) is large and significant: 13.4 percentage points on a base (control group mean) of 35.9 percent. At the 18-month follow-up, the treatment effect point estimate remains positive, but the difference is smaller (6.9 percentage points on a base of 40.9 percent) and no longer statistically significant. Sixty months after random assignment, the treatment and control groups have nearly identical levels of business ownership.[21] These results are not driven by changes in sample composition: we get similar results after restricting the sample to Wave 3 respondents. In all, the results in this first panel

[19] Only 7 GATE participants obtained SBA-backed loans in Wave 1 and 11 in Wave 3.

[20] The ITT estimates are reported in Appendix Table 4 for the main outcomes reported in Table 4. As expected given the noncompliance rates, the point estimates are generally scaled down by a factor of 2 to 3 relative to the LATE estimates reported in Table 4. None of our statistical inferences change. Because most of the LATE estimates are close to zero, the "scaled down" ITT estimates also tend to be close to zero.

[21] We also find very similar average total number of businesses owned between the treatment and control groups over the 60-month sample period.

Table 3—Treatment-Control Differences in Business Practices

Dependent variable	Treatment (1)	Obs. (2)	Control (3)	Obs. (4)	Treatment-control: No covars (5)	Treatment-control: Covariates (6)
Wrote a business plan by W1	0.5000	1,752	0.3725	1,686	0.1275 (0.0168)	0.1276 (0.0172)
Wrote a business plan by W2	0.5974	1,555	0.4666	1,468	0.1308 (0.0180)	0.1296 (0.0185)
Wrote a business plan by W3	0.6761	1,266	0.5662	1,171	0.1100 (0.0196)	0.1108 (0.0200)
Applied for a business loan by W1	0.0592	1,756	0.0627	1,691	−0.0035 (0.0082)	−0.0035 (0.0084)
Applied for a business loan by W2	0.0962	1,560	0.0916	1,473	0.0045 (0.0106)	0.0008 (0.0109)
Applied for a business loan by W3	0.1457	1,270	0.1549	1,175	−0.0092 (0.0145)	−0.0152 (0.0150)

Notes: The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Treatment-control differences with covariates are estimated from a linear probability model that controls for program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

suggest that the positive short-term effects of entrepreneurship training on business ownership fade over time.[22]

Before examining additional outcomes in Table 4, we briefly examine the effects of entrepreneurship training on the dynamics of business entry and exit in Appendix Table 5. Given that the treatment and control groups start with roughly equal ownership rates (Table 1), any differences in business ownership rates at each of the follow-up survey waves are due to differences in business creation rates, differences in business exit rates, or both.[23] The second panel of Appendix Table 5 shows that, conditional on not owning a business at baseline, treatment group members were far more likely to have started a business six months later. This effect dissipates over time. The third panel of Appendix Table 5 shows that, conditional on owning a business at baseline, there are no statistically significant differences in exit rates, although the point estimates are all large and negative.

Overall, the estimates indicate that entrepreneurship training increased average levels of business ownership in the short-run. Entrepreneurship training appears to have drawn new people into starting businesses but did not significantly increase the survival rates of preexisting businesses.[24]

[22] The results are not due to the influence of side or casual businesses, or disguised unemployment (Carter and Sutch 1994). Defining business ownership with 30 or more hours worked per week, we find lower rates of business ownership, but similar treatment-control differences. We also restrict business ownership to only include businesses reporting positive sales at each survey wave to remove nonserious self-employment activities. Again, we find similar results.

[23] See, Evans and Leighton (1989); Fairlie (1999); and Carrasco (1999) for more discussion and empirical estimates of the relationships between self-employment entry, exit and steady-state rates.

[24] Using information on start and stop dates for all businesses owned between survey dates, we find no evidence of treatment effects on total length of time of business ownership.

Table 4—Impact of Entrepreneurship Training on Business Ownership and Main Outcomes

	Treatment-control (IV estimates)		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.1252 (0.0446)	0.1337 (0.0395)	3,443
Business owner at W2 survey date	0.0742 (0.0616)	0.0691 (0.0570)	3,032
Business owner at W3 survey date	0.0406 (0.0844)	0.0105 (0.0810)	2,446
Monthly business sales at W1 survey date (000s)	−1.0817 (0.7510)	−0.9402 (0.7336)	3,210
Monthly business sales at W2 survey date (000s)	−0.6060 (1.1539)	−0.4411 (1.1151)	2,794
Monthly business sales at W3 survey date (000s)	−2.0977 (2.2804)	−2.5522 (2.2885)	2,323
Has any employees at W1 survey date	0.0353 (0.0248)	0.0363 (0.0245)	3,438
Has any employees at W2 survey date	0.0133 (0.0368)	0.0065 (0.0362)	3,023
Has any employees at W3 survey date	−0.0736 (0.0525)	−0.0871 (0.0534)	2,436
log household income at W1	−0.0636 (0.0848)	−0.0223 (0.0639)	3,223
log household income at W2	0.1191 (0.1203)	0.0635 (0.0953)	2,797
log household income at W3	0.2346 (0.1799)	0.0915 (0.1485)	2,270

Notes: The first-stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second-stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

B. *Effects on Business Scale: Sales and Employees*

The dynamics of results on business ownership imply that the marginal businesses produced by entrepreneurship training do not survive in the medium/long-run. Indeed, examining the average treatment effects on business sales and hiring employees, we find no significant effects at any horizon, suggesting that the marginal businesses had low levels of sales and generally did not hire employees (Table 4). Note that these results do not condition on business ownership, and thus capture the treatment's overall impact on sales and hiring employees.

The results for employment do not differ when we change the focus from having an employee to the number of employees (Appendix Table 6). Appendix Table 7 shows the lack of treatment effects on business structure; e.g., on incorporation, or on having a business located outside the home.

TABLE 5—DISTRIBUTION OF BUSINESSES BY ANNUAL SALES AND NUMBER OF EMPLOYEES AT WAVE 3 FOR TREATMENT GROUP, CONTROL GROUP, AND US TOTAL

	Treatment group			Control group			US firms SBO (2007)	
	All individuals	All businesses	New businesses	All individuals	All businesses	New businesses	Total	Started 2002–07
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Annual sales and receipts								
No business	61.1%	NA	NA	62.1%	NA	NA	NA	NA
Less than $5,000	9.7%	24.9%	26.6%	8.9%	23.5%	23.5%	20.6%	22.4%
$5,000 to $9,999	3.8%	9.7%	10.1%	5.0%	13.1%	12.8%	13.7%	13.8%
$10,000 to $24,999	6.5%	16.6%	13.0%	6.8%	18.0%	17.5%	18.8%	18.6%
$25,000 to $49,999	6.7%	17.3%	18.0%	4.6%	12.0%	13.7%	12.1%	12.9%
$50,000 to $99,999	5.6%	14.3%	12.6%	5.7%	15.1%	14.1%	9.9%	10.7%
$100,000 to $249,999	4.5%	11.5%	13.0%	3.8%	9.9%	11.1%	10.2%	10.6%
$250,000 to $499,999	1.5%	3.9%	4.7%	1.6%	4.2%	3.4%	5.5%	5.0%
$500,000 to $999,999	0.4%	0.9%	1.1%	0.7%	1.8%	1.7%	4.0%	3.2%
$1,000,000 or more	0.4%	0.9%	1.1%	0.9%	2.4%	2.1%	5.2%	3.0%
Employment size								
No business	61.1%	NA	NA	62.1%	NA	NA	NA	NA
No employees	29.5%	75.9%	74.0%	26.8%	70.8%	72.2%	81.1%	85.0%
1 to 4 employees	7.2%	18.6%	20.0%	8.6%	22.7%	22.0%	10.6%	10.0%
5 to 9 employees	1.2%	3.1%	3.8%	1.3%	3.4%	2.2%	3.7%	2.6%
10 to 19 employees	0.6%	1.6%	1.6%	0.7%	1.8%	2.2%	2.3%	1.4%
20 to 49 employees	0.2%	0.6%	0.6%	0.2%	0.5%	0.4%	1.4%	0.8%
50 to 99 employees	0.1%	0.2%	0.0%	0.2%	0.5%	0.7%	0.5%	0.2%
100 employees or more	0.0%	0.0%	0.0%	0.2%	0.5%	0.4%	0.4%	0.1%
Sample size	1,116	434	278	1,009	383	234		

Notes: United States total is from the Survey of Business Owners 2007, US Census Bureau, and includes all nonfarm businesses with sales of at least $1,000 in 2007. New businesses are individuals who did not own a business at the time of application to the program.

Do these average treatment effects obscure important effects on the firm size distribution? Table 5 suggests not, focusing on sales and employment at the 60-month follow-up. Column 2 (5) shows sales and employment for treatment group (control group) businesses, and column 3 (6) shows sales and employment for treatment group (control group) businesses created during the study period (we also report the unconditional distributions for all individuals in the treatment and control groups in columns 1 and 4 that are relevant for estimating treatment impacts). Comparing columns 2 and 5 to column 7, our sample has fewer large businesses than the United Stagtes as a whole. This is partly due to the five-year study period: the distributions are more similar when we compare businesses *created* in the past five years (columns 3, 6, and 8). Focusing on the treatment versus control comparisons, we do not find that businesses created by the treatment group are more likely to be successful than businesses created by the control group. In fact, we find that a higher percentage of businesses owned by the *control* group have sales of $500,000 or more. χ^2 tests show that the full treatment and control distributions are not significantly different from each other (column 1 versus column 4).[25]

[25] We also estimate regressions for the probability of creating businesses at various cutoffs above $100,000 and 10 employees using the full Wave 3 sample. We find no treatment effects at any of these cutoffs. We also estimate quantile regressions for sales and employees starting with the seventy-fifth percentile and incrementing up by 5 percentile points to the ninety-fifth percentile. Again, we find no evidence of treatment effects at any of these quantiles. Overall, we do not find evidence that entrepreneurship training increased the likelihood of creating high-revenue or high-employment firms five years post-random assignment.

C. *Average Effects on Earnings: Business and Household*

Training could make businesses more productive even if they do not grow in scale, so we also examine impacts on earnings. The bottom panel of Table 4 shows key summary results: there are no significant effects of training on household income at any horizon, although the confidence intervals include large effects in both directions.[26] Appendix Table 6 unpacks this result by looking at treatment effects on overall employment (wage/salary work or business ownership) and on business performance measured in various ways.[27] We find no evidence of positive effects on business performance; in fact, the point estimates tend to be negative. Appendix Table 8 offers some reassurance that these results are not driven by bias in responding to sensitive or difficult questions on earnings: it shows that item nonresponse for various income measures (and the business sales measure) is not correlated with treatment status.

D. *Nonpecuniary Benefits? Average Effects on Work Satisfaction*

We also investigate the effects of entrepreneurship training on work satisfaction (which we use as a proxy for potential nonpecuniary benefits of self-employment)[28] and find no evidence of significant effects (Appendix Table 6).

E. *Correlations between Entrepreneurship Training and Outcomes in Control Group*

Appendix Table 9 highlights the value of random assignment by providing non-experimental estimates of the "effects" of training. These regressions use only the control group sample, and control for the rich set of baseline characteristics reported in Table 1. The correlations between training receipt and subsequent outcomes in Appendix Table 9 are much larger and more likely to be significant than the treatment effects in Table 4. Even a rich set of controls—baseline household income level, self-employment status, health problems, work experience in a family business, credit history, unemployment insurance receipt, employer-provided health insurance, personality traits, and other standard demographic controls—cannot purge non-experimental treatment effect estimates of strong upward selection bias.

F. *Exploring the Impact of Differential Attrition on the Estimates*

Although we do not find strong evidence of differential attrition based on observables in Section IIE, nor evidence that treatment effect estimates are sensitive to

[26] We also estimate treatment effects on total earnings by combining separately reported business earnings and wage/salary earnings (as opposed to direct reports of total household income). We do not find any significant treatment effects on this measure, nor do we find any significant effects on reliance on public assistance.

[27] The follow-up surveys provide information on (i) how much the owner paid him/herself in regular salary from the business; (ii) how much the owner received in other income payments, such as bonuses, profit distributions, or owner's draw from the business; and (iii) business revenues and expenses. Information is also available on the start and stop dates of all of the businesses owned between each survey wave.

[28] See, for example, Hamilton (2000) and Kawaguchi (2004).

Table 6—Impact of Entrepreneurship Training on Main Outcomes Weighted by Predicted Nonresponse Probabilities

	Treatment-control (IV estimates)		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.0977	0.1141	3,443
	(0.0491)	(0.0425)	
Business owner at W2 survey date	0.0397	0.0689	3,032
	(0.0678)	(0.0607)	
Business owner at W3 survey date	−0.0634	−0.0599	2,446
	(0.0928)	(0.0870)	
Monthly business sales at W1 survey date	−1,525	−1,265	3,210
	(883)	(807)	
Monthly business sales at W2 survey date	−607	−400	2,794
	(1,244)	(1,186)	
Monthly business sales at W3 survey date	−2,868	−2,651	2,323
	(2,267)	(2,152)	
Has any employees at W1 survey date	0.0234	0.0263	3,438
	(0.0269)	(0.0252)	
Has any employees at W2 survey date	−0.0003	0.0007	3,023
	(0.0403)	(0.0379)	
Has any employees at W3 survey date	−0.0739	−0.0794	2,436
	(0.0603)	(0.0580)	
log household income at W1	−0.0419	0.0002	3,223
	(0.0989)	(0.0751)	
log household income at W2	0.1065	0.0561	2,797
	(0.1350)	(0.1084)	
log household income at W3	0.2617	0.1506	2,270
	(0.1947)	(0.1638)	

Notes: The first-stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second-stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance. Sample weights used to estimate treatment-control differences are predicted probabilities of nonresponse in specified wave from first-stage regression using all covariates.

the inclusion of controls for baseline characteristics (Table 4), follow-up survey response rates are higher in the treatment group for each of the follow-up waves, raising the concern that attrition may be correlated with unobserved heterogeneity in outcomes as well. To investigate whether differential attrition might have a large effect on the results, we use two different approaches. First, we estimate regressions for our main set of outcomes using the predicted probability of attrition as a sample weight. The full set of baseline controls are used to estimate these predicted probabilities. This technique places more weight on survivors who look like attriters, in an attempt to compensate for the attriters' absence. The estimates are robust to using these weights (Table 6 versus Table 4).

Second, we conduct a bounds analysis using various assumptions about the treatment effects for attriters, in the spirit of Horowitz and Manski (2000) and Lee (2002; 2009). Table 7, column 4 reproduces the relevant average treatment effect estimate from Table 4. Following Kling et al. (2007) and Karlan and Valdivia (2011), we

Table 7—Impact of Entrepreneurship Training on Main Outcomes: Bounds Analysis

Dependent variable	Lower bounds −0.25 std. (1)	Lower bounds −0.10 std. (2)	Lower bounds −0.05 std. (3)	Unadj. (4)	Upper bounds +0.05 std. (5)	Upper bounds +0.10 std. (6)	Upper bounds +0.25 std. (7)	Standard deviation Control/ treatment (8)
Business owner at W1 survey date	0.0218	0.0892	0.1116	0.1337	0.1565	0.1790	0.2463	0.4799
	(0.0333)	(0.0331)	(0.0330)	(0.0395)	(0.0330)	(0.0331)	(0.0332)	0.4911
Business owner at W2 survey date	−0.1528	−0.0170	0.0282	0.0691	0.1187	0.1640	0.2997	0.4918
	(0.0425)	(0.0421)	(0.0421)	(0.0570)	(0.0421)	(0.0421)	(0.0425)	0.4953
Business owner at W3 survey date	−0.3921	−0.1384	−0.0539	0.0105	0.1152	0.1998	0.4534	0.4854
	(0.0491)	(0.0486)	(0.0485)	(0.0810)	(0.0485)	(0.0486)	(0.0492)	0.4877
Monthly business sales at W1 survey date (000s)	−3.3615	−1.9122	−1.4291	−0.9402	−0.4629	0.0202	1.4695	9.1599
	(0.5717)	(0.5680)	(0.5675)	(0.7336)	(0.5675)	(0.5680)	(0.5716)	6.5686
Monthly business sales at W2 survey date (000s)	−5.6119	−2.5024	−1.4659	−0.4411	0.6071	1.6436	4.7531	9.7962
	(0.7856)	(0.7782)	(0.7771)	(1.1151)	(0.7771)	(0.7781)	(0.7854)	8.8079
Monthly business sales at W3 survey date (000s)	−14.2217	−7.0030	−4.5968	−2.5522	0.2157	2.6219	9.8405	13.8608
	(1.3269)	(1.3126)	(1.3106)	(2.2885)	(1.3108)	(1.3131)	(1.3282)	11.9129
Has any employees at W1 survey date	−0.0254	0.0121	0.0246	0.0363	0.0496	0.0622	0.0997	0.2589
	(0.0201)	(0.0200)	(0.0200)	(0.0245)	(0.0200)	(0.0200)	(0.0201)	0.2793
Has any employees at W2 survey date	−0.1250	−0.0434	−0.0162	0.0065	0.0382	0.0654	0.1470	0.2918
	(0.0265)	(0.0263)	(0.0263)	(0.0362)	(0.0263)	(0.0263)	(0.0265)	0.2972
Has any employees at W3 survey date	−0.3404	−0.1815	−0.1286	−0.0871	−0.0226	0.0303	0.1892	0.3135
	(0.0316)	(0.0313)	(0.0313)	(0.0534)	(0.0313)	(0.0313)	(0.0317)	0.2907
log household income at W1	−0.2736	−0.1111	−0.0570	−0.0223	0.0513	0.1055	0.2679	0.9111
	(0.0541)	(0.0536)	(0.0535)	(0.0639)	(0.0535)	(0.0536)	(0.0541)	0.8992
log household income at W2	−0.3726	−0.0611	0.0428	0.0635	0.2505	0.3543	0.6658	0.9434
	(0.0705)	(0.0698)	(0.0697)	(0.0953)	(0.0698)	(0.0699)	(0.0708)	0.9425
log household income at W3	−0.7005	−0.1332	0.0558	0.0915	0.4340	0.6230	1.1903	1.0113
	(0.0883)	(0.0871)	(0.0870)	(0.1485)	(0.0871)	(0.0873)	(0.0888)	0.9596

Notes: See Table 4 for notes and sample sizes. Columns 1 and 7 impute to the lower (upper) bound the mean minus (plus) 0.25 standard deviations of the observed treatment distribution to the nonresponders in the treatment group and the mean plus (minus) 0.25 standard deviations of the observed control distribution to nonresponders in the control group. Columns 2, 3, 5, and 6 repeat the exercise subtracting and adding the specified standard deviations. Column 4 (unadjusted) reproduces the estimates reported in Table 4.

impute to the lower (upper) bound the mean minus (plus) a specified standard deviation multiple of the observed treatment group distribution to the nonresponders in the treatment group, and the mean plus (minus) the same standard deviation multiple of the observed control group distribution to nonresponders in the control group. In column 3, for example, we create a conservative treatment effect estimate by assuming that treatment group attriters have the mean value for the dependent variable minus 0.05 standard deviations among nonattriting treatment observations, and that the control group attriters have the mean value for the dependent variable plus 0.05 standard deviations among the nonattriting control observations.

Table 7 indicates that the results are not overly sensitive to adding and subtracting 0.05 standard deviations from the means, but are sensitive to moving 0.25 standard deviations from the means (columns 1 and 7). To put the magnitudes of these changes in perspective, Table 7 also reports the control and treatment standard deviations in column 8 (the treatment and control means are reported in Table 4). For business ownership at Wave 1, for example, the −0.05 adjustment reported in column 4 assumes that the attriting treatment group has a 2.5 percentage point lower business ownership rate than the nonattriting treatment sample, and that the attriting

control group has a 2.4 percentage point higher business ownership rate than the nonattriting control sample. These are large changes from a base business ownership rate of roughly 35 to 40 percent but do not result in major changes in the results.[29]

If we focus on the disappearance of the 13 percentage point short-run treatment effect by the 60-month follow-up survey, we find it would take an extreme form of biased attrition to regenerate the treatment effect in the long run. For the treatment effect to be 13 percentage points at the 60-month follow-up, it would require the attritors in the treatment group to have at least a 0.10 standard deviation higher business ownership rate than nonattritors and attritors in the control group to have at least a 0.10 standard deviation lower business ownership rate than nonattritors.

Columns 5–7 of Table 7 also show the particular and strong form that attrition would need to take to create positive effects on outcomes other than short-run business ownership and employment. It would have to be the case that treatment group attritors have substantially more positive treatment effects, and/or that control group attritors have substantially more negative treatment effects than nonattritors. In all, the results in Tables 6 and 7 suggest that attrition would have to be particularly strongly correlated with treatment effects to change inferences based on our main results.

G. *Comparison to Findings from Evaluation Report*

Our methodology, results, and inferences differ from the final evaluation report delivered to the US Department of Labor (Benus et al. 2009). Methodologically: (i) we include additional outcomes beyond those reported in the original evaluation report; (ii) we estimate LATE as well as ITT because of noncompliance in both treatment and control; (iii) we estimate distributional effects on business sales and employment; (iv) we test several hypotheses regarding the rationales for training interventions by estimating heterogeneous treatment effects; (v) we do not remove observations if the business has a business partnership with another study participant with the opposite treatment assignment (120 study participants in 56 business partnerships); (vi) our treatment effect estimates for business outcomes include (as zeros) those without a business; (vii) we address differential attrition between the treatment and control groups using bounds analysis rather than weighting; and (viii) we do not use a hot-deck procedure to impute missing values for outcome measures and instead exclude observations with missing values for the dependent variable.

The methodological differences produce different results. The final evaluation report finds positive estimates for total business earnings except for Wave 1, and a total treatment/control difference of $1,128 from combining all waves (although the difference is not statistically significant). Our analysis of the GATE Project data, however, provides no evidence of a positive business earnings treatment effect (in

[29] We also estimate bounds using the trimming procedure suggested in Lee (2002; 2009). The estimated range is similar to that reported for 0.10 standard deviations for most outcome measures.

fact, the point estimates for each wave are negative).[30] We also do not find any treatment effects on the hiring of employees, which differs from the final evaluation report's conclusion that the program generated additional jobs beyond those of the owners. We examined treatment effects on whether any employees are hired, number of employees and the full distribution of hiring employees, and find no effects.[31] The final evaluation report also notes that businesses created by the treatment group had greater longevity than businesses created by the control group. In contrast, we do not find evidence that training lowers exit rates or increases likelihoods of being in business at follow-up for those who were business owners at baseline, nor do we find that training increases the total length of time of business ownership.

The different results lead to different inferences about the (cost-) effectiveness of the GATE program. The final evaluation report concludes "that the benefits of Project GATE exceed its costs," [32] and "DOL should initiate a new self-employment training program similar to Project GATE in all states."[33] Our findings, both above with respect to the average effects of the program and below with respect to groups that might be particularly vulnerable to market failures, provide little support for the hypotheses that GATE is cost-effective and/or a relatively efficient way to mitigate market failure(s).

IV. Hypothesis Testing Based on Heterogeneous Treatment Effects

To shed light on various rationales for training subsidies, we next explore heterogeneous treatment effects. We estimate these effects by adding several interactions between key baseline characteristics and treatment status to our model of conditional average treatment effects (i.e., we estimate each heterogeneous effect of interest conditional on the others). Table 8A reports estimates for our main outcomes. Each row presents results from a single regression. We also estimate average treatment effects on subsamples of key groups to address the policy question of whether training benefits targeted groups in level if not relative terms (Table 8B).

Credit/liquidity constraints are one important rationale for training subsidies: constraints may prevent potential entrepreneurs from obtaining training, even if training is valuable.[34] Alternatively, or possibly additionally, training may help recipients relax liquidity constraints by helping them find alternative sources of financing (e.g., microlenders, SBA lenders, Community Development Financial Institutions, etc.)

[30] An Appendix table in the final evaluation report sheds some light on the potential causes of the discrepancy. It shows that the business earnings treatment effect estimates are sensitive to hot-decking, excluding treatment/control partnerships, and sample weighting. When each of these procedures is separately removed the positive total business earnings estimate becomes noticeably smaller.

[31] The discrepancy here appears to be due to our focus on changes per individual instead of total counts. The latter are partly influenced by higher response rates among the treatment group.

[32] The actual estimated benefit/cost to society calculated in the report is −$1,891, but arguments are made that underreporting of business earnings could make the estimated positive business earnings effect larger, and that higher treatment group job creation (which is not included in the calculation) could improve the final benefit/cost estimate (Benus et al. 2009).

[33] The Department of Labor recently funded a new round of GATE programs in three new states and one previous state "because of the success of the original Project GATE" (US Department of Labor 2010).

[34] See Parker (2009), Fairlie and Krashinsky (2012), and Kerr and Nanda (2011) for recent reviews of the literature on credit constraints for entrepreneurs.

Table 8A—Entrepreneurship Training Impact Heterogeneity by Baseline Characteristics

		Credit constraints	Discrimination		Human capital constraints			
Dependent variable	Main treatment (1)	Bad credit × treatment (2)	Minority × treatment (3)	Female × treatment (4)	No college × treatment (5)	No managerial exp. × treat. (6)	Did not work in fam. bus. × treat. (7)	No prior business exp. × treat. (8)
Business owner at W1 survey date	0.1136	0.0017	−0.0184	−0.0915	0.0125	−0.0901	−0.0030	−0.0066
	(0.1010)	(0.0746)	(0.0535)	(0.0708)	(0.0781)	(0.0747)	(0.0781)	(0.0814)
Business owner at W2 survey date	−0.0119	0.0666	−0.0758	−0.0157	0.0486	0.0431	0.0622	−0.1097
	(0.1390)	(0.1028)	(0.0599)	(0.0975)	(0.1100)	(0.1034)	(0.1066)	(0.1102)
Business owner at W3 survey date	0.1357	−0.0800	−0.0857	0.0405	0.0433	0.1772	−0.0809	−0.0320
	(0.1821)	(0.1309)	(0.0623)	(0.1268)	(0.1388)	(0.1349)	(0.1399)	(0.1419)
Monthly business sales at W1 survey date (000s)	−1.6755	−0.4427	0.4240	0.7087	1.3018	0.0042	−1.0323	−1.0629
	(3.5875)	(1.2731)	(1.0181)	(1.3686)	(1.6821)	(1.3375)	(1.9047)	(1.5851)
Monthly business sales at W2 survey date (000s)	−3.5339	4.9239	0.3104	−1.8620	0.5023	0.6448	2.1767	−1.0231
	(5.0097)	(2.7786)	(0.9752)	(2.6046)	(3.0595)	(2.3753)	(2.8692)	(2.5277)
Monthly business sales at W3 survey date (000s)	−9.5648	−1.2386	2.5551	0.6067	6.8759	−1.0964	3.6547	−5.5627
	(7.6381)	(4.3138)	(2.1657)	(3.7112)	(5.7169)	(4.2456)	(5.1034)	(4.9274)
Has any employees at W1 survey date	−0.1063	0.0493	0.0005	−0.0046	0.0575	0.0073	0.1563	0.0030
	(0.0689)	(0.0482)	(0.0325)	(0.0443)	(0.0497)	(0.0456)	(0.0503)	(0.0519)
Has any employees at W2 survey date	−0.1347	−0.0092	0.0073	0.0114	0.0594	0.0272	0.0410	0.0400
	(0.0952)	(0.0686)	(0.0399)	(0.0620)	(0.0718)	(0.0656)	(0.0707)	(0.0710)
Has any employees at W3 survey date	−0.2165	−0.1067	−0.0330	−0.0471	0.1811	0.0012	0.0651	0.1514
	(0.1215)	(0.0865)	(0.0444)	(0.0786)	(0.0903)	(0.0847)	(0.0910)	(0.0901)
log household income at W1	−0.0298	0.0827	0.1533	0.0553	0.0954	−0.2044	−0.0857	−0.0459
	(0.1682)	(0.1224)	(0.0842)	(0.1155)	(0.1214)	(0.1219)	(0.1235)	(0.1272)
log household income at W2	−0.0520	0.2167	−0.0061	−0.0581	−0.0788	−0.0174	0.1096	−0.0418
	(0.2450)	(0.1848)	(0.1170)	(0.1690)	(0.1923)	(0.1861)	(0.1844)	(0.1878)
log household income at W3	−0.0665	−0.1696	0.1455	−0.1582	0.1081	0.2505	0.2065	0.0258
	(0.3592)	(0.2569)	(0.1183)	(0.2385)	(0.2635)	(0.2441)	(0.2637)	(0.2683)

(*Continued*)

and navigate application processes. Part of the coursework and advising in entrepreneurship training is devoted to providing information and assistance in finding capital. If either of these mechanisms is in play, then we might expect subsidized training to have (relatively) strong, positive effects on the credit-constrained, conditional on other characteristics.

Our measure of baseline credit constraints comes from the application question: "Do you have any problems with your credit history?" We construct a dummy that takes a value of 1 for the 44 percent of the sample that responded "yes," and either interact this dummy with treatment status (Table 8A, column 2) or limit the sample to those with credit problems (Table 8B, column 1). We do not find evidence that training has positive effects on the credit-constrained: across the two tables we find only 1 significant point estimate out of 30 (for business ownership in Wave 1). We also estimate whether entrepreneurship training differentially affects the level of invested capital in the business for those with credit problems. We do not find any evidence that training affects investment, debt, or loan applications, overall or differentially for the credit-constrained.

Labor market discrimination is another potential rationale for training subsidies: if employers discriminate more than customers, then low-cost training may be a

Table 8A—Entrepreneurship Training Impact Heterogeneity by Baseline Characteristics (*Continued*)

		U.I. frictions
Dependent variable	Main treatment (9)	Unemployed × treatment (10)
Business owner at W1 survey date	0.1136	0.1777
	(0.1010)	(0.0775)
Business owner at W2 survey date	−0.0119	0.1387
	(0.1390)	(0.1059)
Business owner at W3 survey date	0.1357	−0.1396
	(0.1821)	(0.1356)
Monthly business sales at W1 survey date (000s)	−1.6755	1.7888
	(3.5875)	(1.4655)
Monthly business sales at W2 survey date (000s)	−3.5339	0.9719
	(5.0097)	(2.5009)
Monthly business sales at W3 survey date (000s)	−9.5648	6.8971
	(7.6381)	(4.6315)
Has any employees at W1 survey date	−0.1063	−0.0229
	(0.0689)	(0.0454)
Has any employees at W2 survey date	−0.1347	0.0855
	(0.0952)	(0.0650)
Has any employees at W3 survey date	−0.2165	−0.0368
	(0.1215)	(0.0839)
log household income at W1	−0.0298	−0.0282
	(0.1682)	(0.1213)
log household income at W2	−0.0520	0.0071
	(0.2450)	(0.1792)
log household income at W3	−0.0665	−0.0399
	(0.3592)	(0.2587)

Notes: Each row represents a separate regression. Sample sizes are reported in Table 4. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

relatively efficient method for redistributing to affected groups.[35] We present treatment effect interactions for minorities and females in Table 8A, columns 3 and 4, and estimate separate regressions for minority and female subsamples in Table 8B, columns 2 and 3. We do not find evidence that training has relatively strong or lasting effects for minorities or women. In fact, the point estimates for business ownership are negative for women at 6 and 18 months, producing overall effects on business ownership for the female subsample that are not statistically significant (Table 8B, column 3).

Human and managerial capital constraints are another important rationale for training subsidies: if education or managerial labor markets do not function well,

[35] See Borjas and Bronars (1989) and Fairlie and Robb (2008) for a discussion of customer and other forms of discrimination against minority business owners, and Altonji and Blank (1999) for a review of the larger literature on racial and gender discrimination in the labor market.

Table 8B—Separate Entrepreneurship Training Impact Regressions for Subgroups

	Credit constraints	Discrimination		Human capital constraints				UI frictions
Dependent variable	Bad credit (1)	Minority (2)	Female (3)	No college (4)	No manager. exp. (5)	Did not work in fam. bus. (6)	No prior business exp. (7)	Unemployed (8)
Business owner at W1 survey date	0.1209	0.0845	0.0317	0.1316	0.0812	0.1428	0.1613	0.2284
	(0.0588)	(0.0646)	(0.0625)	(0.0518)	(0.0686)	(0.0487)	(0.0513)	(0.0567)
Business owner at W2 survey date	0.0261	−0.0171	−0.0510	0.0621	0.1099	0.0730	0.0450	0.1035
	(0.0680)	(0.0741)	(0.0700)	(0.0593)	(0.0776)	(0.0550)	(0.0578)	(0.0634)
Business owner at W3 survey date	−0.0849	−0.0571	0.0061	−0.0270	0.1290	0.0214	0.0384	0.0007
	(0.0797)	(0.0898)	(0.0814)	(0.0690)	(0.0906)	(0.0620)	(0.0645)	(0.0705)
Monthly business sales at W1 survey date (000s)	−0.4417	0.1804	−1.0492	−0.7884	−0.6066	−1.8038	−0.6287	0.1271
	(0.9823)	(1.0300)	(0.9957)	(1.2675)	(1.3648)	(1.0789)	(1.0550)	(1.0673)
Monthly business sales at W2 survey date (000s)	2.4018	0.7291	−1.8542	−0.6780	1.1385	0.6934	−0.9273	−0.1807
	(1.9300)	(1.0475)	(1.2998)	(1.4052)	(1.6936)	(1.4469)	(1.0897)	(1.2290)
Monthly business sales at W3 survey date (000s)	1.2806	0.9949	−0.2497	−0.0367	−0.8944	−0.5805	−1.2355	0.9200
	(1.4889)	(2.0189)	(1.0795)	(2.2003)	(1.8664)	(1.8115)	(1.3271)	(1.6952)
Has any employees at W1 survey date	0.0430	0.0781	0.0077	0.0374	0.1004	0.0785	0.0476	0.0442
	(0.0447)	(0.0463)	(0.0455)	(0.0407)	(0.0491)	(0.0360)	(0.0306)	(0.0370)
Has any employees at W2 survey date	0.0442	0.0110	−0.0247	0.0131	0.0911	0.0100	0.0031	0.0098
	(0.0514)	(0.0545)	(0.0491)	(0.0463)	(0.0551)	(0.0389)	(0.0360)	(0.0411)
Has any employees at W3 survey date	−0.0337	−0.0213	−0.0427	0.0147	0.0731	−0.0125	−0.0060	−0.0184
	(0.0527)	(0.0608)	(0.0511)	(0.0476)	(0.0614)	(0.0405)	(0.0389)	(0.0447)
log household income at W1	0.0840	0.0986	−0.0467	−0.0505	−0.1849	−0.1971	−0.2262	−0.2502
	(0.1406)	(0.1739)	(0.1363)	(0.1204)	(0.1609)	(0.1001)	(0.1015)	(0.1110)
log household income at W2	0.1169	−0.0658	0.2654	0.0231	0.0840	−0.0304	−0.0081	−0.0359
	(0.1592)	(0.1749)	(0.1465)	(0.1218)	(0.1775)	(0.1084)	(0.1089)	(0.1222)
log household income at W3	0.0828	0.1197	0.0548	0.1043	0.2608	0.1284	0.0805	0.1533
	(0.1783)	(0.2044)	(0.1694)	(0.1464)	(0.1856)	(0.1239)	(0.1346)	(0.1402)
W1 sample size	1,491	1,448	1,636	2,100	1,268	2,355	2,138	1,870
W2 sample size	1,265	1,217	1,454	1,804	1,097	2,077	1,861	1,639
W3 sample size	958	915	1,167	1,382	844	1,686	1,484	1,335

Notes: The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance. Each row/column represents a separate regression.

then subsidizing training may improve efficiency or efficiently redistribute to the most affected parties. Self-employment training may be especially helpful to those lacking the main human capital factors found to be associated with business success in the previous literature: education, previous managerial experience, and previous experience working in a family business.[36] However, estimates reported in Table 8A, columns 5–8 and Table 8B, columns 4–7 do not provide evidence that training has relatively lasting or strong effects on those with less education, less previous managerial experience, less experience working in a family business, or less prior experience owning a business.

[36] See Parker (2009), Fairlie and Robb (2008), and Van Praag (2005) for reviews of this literature.

Unemployment insurance frictions are perhaps the most important, or at least most commonly invoked, rationale for training subsidies. Entrepreneurship training may be a relatively efficient way to insure against job loss by providing recipients with incentives to work by creating a job for themselves (and perhaps others).[37] We test this by interacting treatment status with a measure of baseline unemployment. We find that those unemployed at baseline are in fact more likely to have a business at the six-month follow-up (Table 8A, column 8 and Table 8B, column 7). This effect disappears at later follow-ups, however: we find no effects in the longer-run. Nor do we find any other evidence of strong or lasting effects for the unemployed, in either relative (Table 8A) or absolute (Table 8B) terms. These results in Table 8A do not change if we compare the unemployed only to wage/salary workers, dropping those who were business owners at baseline.[38]

We also estimate treatment effect heterogeneity based on business ownership (yes/no) at baseline. These results show little evidence that training is effective for those who owned or did not own businesses when entering training, or relatively effective for nonbusiness owners when entering training (Appendix Table 10).

V. Conclusion

Although substantial resources are devoted to subsidizing entrepreneurship training around the world, we know very little about its effectiveness and whether it alleviates market frictions. We provide new estimates of average and heterogeneous treatment effects of entrepreneurship training from Project GATE. We find evidence that training increases business planning and business ownership in the short-run, but that the marginal businesses are unsuccessful and fail to produce tangible or subjective benefits at any of the three follow-up horizons (6-, 18-, and 60-months). We do not find any evidence that training shifts the distribution of firms in important ways (e.g., by disproportionately creating very successful firms) that might be missed by analysis of average treatment effects. Although we find higher attrition among the control group, bounds analyses confirm that only extreme forms of biased attrition would change these results.

Our analysis of treatment heterogeneity produces some novel insights about the theory and design of training interventions. Many of the rationales put forward for subsidizing training—countering credit or human capital constraints in enterprise development, or labor market discrimination—are not borne out by the data. We do

[37] Another explanation for why the unemployed may benefit more from job training is that they have more time to devote to it. But we do not find any evidence that the unemployed (at baseline) receive more or different training.

[38] We define the unemployed to include anyone who is not working in a wage/salary job or self-employed at the time of application. Participating in the GATE program implies some level of interest in work, and our definition facilitates a straightforward classification of the sample into the two main categories of unemployment and employment (i.e., wage/salary work or self-employment). We find similar results when using alternative definitions of unemployment. First, we estimate both sets of regressions using UI recipients (which was used in the final evaluation report, Benus et al. 2009). Second, we estimate regressions using a slightly more restrictive definition of unemployment to include only those "looking for work" at the time of application. This is the definition used in Michaelides and Benus (2010) which builds on the final evaluation report by shifting the focus from UI recipients to the unemployed. They find stronger positive estimates of treatment effects for the unemployed than those reported for UI recipients in the final evaluation report. Under any and all definitions, we find positive effects on business ownership in the short-run, but no effects on any outcomes in the long run.

find evidence that entrepreneurship training has relatively strong positive effects on business ownership for the unemployed in the short run, but these effects disappear by the long run.

In all, the absence of positive effects of entrepreneurship training across numerous measures of business ownership, business performance and broader outcomes, and the estimated $1,321 per recipient cost of providing GATE training, suggests that entrepreneurship training may not be a cost-effective method of addressing credit, human capital, discrimination, or social insurance constraints. This conclusion contrasts with the positive benefit/cost conclusion reached in the final evaluation report submitted to DOL (Benus et al. 2009), and with similarly positive arguments proffered by advocates of state-level programs.[39] It also contrasts with the more positive findings related to the medium- and long-term effects of job training on labor market outcomes (Card, Kluve, and Weber 2010; Osikominu 2013).

Understanding more about the effects and mechanisms of entrepreneurship training is important given the continued growth and popularity of these programs around the world. Many financial institutions with a social aim now bundle business training with their loans. The Department of Labor recently funded a new round of GATE programs in four additional states based on the findings from the GATE Project, and President Barack Obama recently signed the Small Business Jobs Act, which expands funding to SBDCs throughout the country. Individual states also continue to extend Self-Employment Assistance (SEA) programs that were originally authorized by Congress in 1993 and made permanent in 1997.

Our results demonstrate the importance of understanding which components of training are more and less helpful, for which populations, and whether training might (only) be effective when bundled with complementary interventions. Many questions ensue, for example, should subsidies for entrepreneurship training be re-allocated to job training? Should content from entrepreneurship training be grafted onto job training? Are there groups thus far not identified for whom entrepreneurship training may be beneficial in the longer run? Would the effects of training be stronger if they were combined with greater availability financial capital, rather than merely providing assistance in applying for existing options?[40]

[39] For example, the New York Senate (2011) justified extending the SEA program by stating that it "has been extremely successful in helping individuals who are likely to exhaust their regular unemployment insurance benefits to develop and establish small businesses in New York … The success of this program is evident. Over 4,000 jobs have been created and $16 million in state tax revenue has been generated at no cost to the state."

[40] Recent evaluations of entrepreneurship training in developing countries have yielded mixed results, and similar questions of pedagogy, integration with other services, content, and market contexts persist. See McKenzie and Woodruff (2012) for a discussion and meta-analysis of these developing country studies.

Appendix

Appendix Table 1A—Impact of Entrepreneurship Training for Pittsburgh and Duluth Sites (*with 94% Small Business Development Center Representation*) Compare to Table 4

	Treatment-control (IV estimates)		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.0928	0.1093	648
	(0.0880)	(0.0787)	
Business owner at W2 survey date	0.0531	0.0624	576
	(0.1249)	(0.1127)	
Business owner at W3 survey date	−0.0184	−0.0279	475
	(0.1768)	(0.1643)	
Monthly business sales at W1 survey date (000s)	−1.1131	−0.1398	604
	(1.6308)	(1.3387)	
Monthly business sales at W2 survey date (000s)	−3.8658	−2.6955	524
	(2.4441)	(2.1200)	
Monthly business sales at W3 survey date (000s)	−6.7211	−4.5823	451
	(4.3437)	(3.2430)	
Has any employees at W1 survey date	0.0194	0.0220	646
	(0.0454)	(0.0443)	
Has any employees at W2 survey date	−0.0173	−0.0143	573
	(0.0721)	(0.0692)	
Has any employees at W3 survey date	−0.1237	−0.1114	473
	(0.1142)	(0.1112)	
log household income at W1	−0.1393	−0.0387	591
	(0.1774)	(0.1382)	
log household income at W2	0.2511	0.3699	513
	(0.2699)	(0.2184)	
log household income at W3	−0.3520	−0.2129	432
	(0.4113)	(0.3283)	

Notes: The first stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 1B—Impact of Entrepreneurship Training for Philadelphia Site
(*100% Community Based Organization Representation*)
Compare to Table 4

	Treatment-control (IV estimates)		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.0372	0.1014	903
	(0.0814)	(0.0740)	
Business owner at W2 survey date	−0.0583	−0.0375	745
	(0.1167)	(0.1103)	
Business owner at W3 survey date	−0.0786	−0.1750	551
	(0.1904)	(0.1981)	
Monthly business sales at W1 survey date (000s)	−0.9343	−0.1970	843
	(1.2288)	(1.2704)	
Monthly business sales at W2 survey date (000s)	0.8949	1.1190	693
	(1.0799)	(1.2526)	
Monthly business sales at W3 survey date (000s)	−0.0563	0.0693	519
	(1.0094)	(1.0611)	
Has any employees at W1 survey date	−0.0271	−0.0155	901
	(0.0468)	(0.0465)	
Has any employees at W2 survey date	−0.0869	−0.0590	741
	(0.0747)	(0.0742)	
Has any employees at W3 survey date	−0.0147	0.0044	549
	(0.1252)	(0.1335)	
log household income at W1	−0.0353	0.1894	823
	(0.1735)	(0.1440)	
log household income at W2	−0.1082	0.0216	680
	(0.2375)	(0.2038)	
log household income at W3	0.5542	0.8075	507
	(0.4262)	(0.4062)	

Notes: The first stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 2—Regressions for Probability of Receiving Entrepreneurship Training

	W1 (1)	W2 (2)	W3 (3)
Female	0.0242	0.0410	0.0413
	(0.0259)	(0.0276)	(0.0295)
Black	0.0843	0.0694	0.1123
	(0.0365)	(0.0403)	(0.0446)
Latino	0.0688	0.0878	0.1027
	(0.0628)	(0.0643)	(0.0642)
Asian	−0.1439	−0.0679	−0.0730
	(0.0846)	(0.0988)	(0.1129)
Other	0.0072	0.0260	0.1006
	(0.0487)	(0.0536)	(0.0550)
Not US born	0.0706	0.0498	0.0719
	(0.0523)	(0.0564)	(0.0582)
Age	0.0100	0.0056	0.0017
	(0.0098)	(0.0105)	(0.0113)
Age squared	−0.0001	0.0000	0.0000
	(0.0001)	(0.0001)	(0.0001)
Married	0.0202	0.0308	0.0485
	(0.0306)	(0.0334)	(0.0365)
Has children	0.0267	0.0154	0.0327
	(0.0286)	(0.0311)	(0.0331)
Highest grade completed	0.0213	0.0393	0.0446
	(0.0112)	(0.0123)	(0.0135)
College education	0.0136	−0.0304	−0.0652
	(0.0498)	(0.0540)	(0.0570)
HH Income $25,000–49,999	0.0232	0.0250	0.0709
	(0.0326)	(0.0359)	(0.0390)
HH Income $50,000–74,999	0.0185	0.0199	0.0705
	(0.0414)	(0.0456)	(0.0498)
HH Income $75,000–99,999	0.0808	0.1354	0.2253
	(0.0577)	(0.0594)	(0.0599)
HH Income $100,000+	0.0917	0.0795	0.0980
	(0.0565)	(0.0603)	(0.0651)
Wage/salary work	−0.0133	−0.0089	−0.0311
	(0.0380)	(0.0425)	(0.0453)
Self-employed with no employees	0.0795	0.0867	0.0274
	(0.0460)	(0.0457)	(0.0493)
Self-employed with employees	0.0118	0.0315	0.0014
	(0.0455)	(0.0472)	(0.0503)
Has a health problem	0.0037	−0.0063	−0.0032
	(0.0470)	(0.0520)	(0.0586)
Has relatives or friends who have been previously S.E.	0.0406	0.0216	0.0270
	(0.0319)	(0.0341)	(0.0370)
Ever worked for relatives or friends who are S.E.	0.0078	0.0248	0.0410
	(0.0306)	(0.0329)	(0.0348)
Has a bad credit history	−0.0395	0.0277	0.0369
	(0.0293)	(0.0318)	(0.0346)
Currently receiving UI benefits	−0.0386	−0.0297	−0.0610
	(0.0292)	(0.0312)	(0.0330)
Has health insurance from current employer	−0.0586	−0.0900	−0.0707
	(0.0385)	(0.0426)	(0.0462)
Autonomy index	0.0066	−0.0156	−0.0078
	(0.0119)	(0.0131)	(0.0139)
Risk tolerance index	0.0174	0.0176	0.0404
	(0.0127)	(0.0140)	(0.0150)
Managerial experience	0.0142	0.0455	0.0554
	(0.0279)	(0.0299)	(0.0327)
Treatment	0.1198	0.2980	0.4986
	(0.3111)	(0.3319)	(0.3616)

(*Continued*)

Appendix Table 2—Regressions for Probability of Receiving Entrepreneurship Training (*Continued*)

	W1 (1)	W2 (2)	W3 (3)
Female × treatment	−0.0216	−0.0557	−0.0635
	(0.0327)	(0.0333)	(0.0352)
Black × treatment	−0.0726	−0.0443	−0.0424
	(0.0475)	(0.0500)	(0.0546)
Latino × treatment	−0.0673	−0.0716	−0.0872
	(0.0765)	(0.0754)	(0.0770)
Asian × treatment	0.1176	0.0108	0.0860
	(0.1081)	(0.1202)	(0.1342)
Other × treatment	0.0194	0.0126	−0.0407
	(0.0612)	(0.0631)	(0.0629)
Not US born × treatment	−0.1275	−0.0941	−0.1264
	(0.0683)	(0.0703)	(0.0763)
Age × treatment	0.0090	0.0069	0.0040
	(0.0120)	(0.0125)	(0.0134)
Age squared × treatment	−0.0001	−0.0001	−0.0001
	(0.0001)	(0.0001)	(0.0002)
Married × treatment	0.0040	−0.0086	−0.0441
	(0.0385)	(0.0401)	(0.0429)
Has children × treatment	−0.0196	0.0039	0.0161
	(0.0358)	(0.0369)	(0.0390)
Highest grade completed × treatment	0.0109	0.0003	−0.0124
	(0.0144)	(0.0150)	(0.0163)
College education × treatment	−0.0352	−0.0327	0.0356
	(0.0624)	(0.0642)	(0.0678)
HH Income $25,000–49,999 × treatment	−0.0554	−0.0262	−0.0829
	(0.0424)	(0.0443)	(0.0472)
HH Income $50,000–74,999 × treatment	−0.0216	−0.0321	−0.0875
	(0.0518)	(0.0547)	(0.0581)
HH Income $75,000–99,999 × treatment	−0.0813	−0.1646	−0.2571
	(0.0708)	(0.0720)	(0.0722)
HH Income $100,000+ × treatment	−0.0573	−0.0680	−0.0968
	(0.0677)	(0.0708)	(0.0751)
Wage/salary work × treatment	−0.0189	−0.0173	0.0127
	(0.0482)	(0.0508)	(0.0537)
Self-employed with no employees × treatment	−0.0613	−0.0610	−0.0432
	(0.0557)	(0.0539)	(0.0584)
Self-employed with employees × treatment	0.0488	0.0310	0.0283
	(0.0545)	(0.0533)	(0.0569)
Has a health problem × treatment	0.0285	0.0052	0.0059
	(0.0585)	(0.0617)	(0.0682)
Has relatives or friends who have been previously S.E. × treatment	−0.0264	−0.0083	−0.0123
	(0.0404)	(0.0414)	(0.0447)
Ever worked for relatives or friends who are S.E. × treatment	−0.0099	−0.0086	−0.0060
	(0.0377)	(0.0386)	(0.0402)
Has a bad credit history × treatment	0.0457	0.0010	−0.0225
	(0.0367)	(0.0377)	(0.0404)
Currently receiving UI benefits × treatment	0.0552	0.0533	0.0827
	(0.0373)	(0.0380)	(0.0399)
Has health insurance from current employer × treatment	0.1029	0.1439	0.1085
	(0.0484)	(0.0503)	(0.0537)
Autonomy index × treatment	−0.0046	0.0166	0.0137
	(0.0155)	(0.0163)	(0.0175)
Risk tolerance index × treatment	−0.0091	−0.0094	−0.0325
	(0.0164)	(0.0169)	(0.0180)
Managerial experience × treatment	0.0302	−0.0100	−0.0212
	(0.0351)	(0.0362)	(0.0392)

Notes: All reported characteristics are measured at time of application, prior to random assignment. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application.

Appendix Table 3—Self-Reported Amount that Entrepreneurship Training Helped Recipients in Various Ways

	Very useful	Somewhat useful	Not very useful	Not at all useful
How would you rate the overall usefulness of the services you have received?				
Treatment group	51.7%	33.7%	8.5%	6.1%
Control group	35.8%	40.8%	10.8%	12.7%

	Treatment group			Control group		
GATE Services	A lot	Somewhat	Not at all	A lot	Somewhat	Not at all
Helped with applying for loans	12.6%	21.5%	65.9%	5.9%	17.2%	76.8%
Helped with deciding whether to pursue self. emp.	39.5%	33.1%	27.4%	23.6%	30.0%	46.4%
Helped with refining the business idea	34.1%	37.2%	28.8%	23.0%	32.3%	44.7%
Helped with credit issues	16.4%	25.8%	57.7%	10.9%	17.3%	71.7%
Helped with developing a marketing strategy	31.4%	37.4%	31.2%	19.6%	31.6%	48.8%
Helped with legal issues	19.3%	35.5%	45.2%	11.3%	28.2%	60.6%
Helped with accounting issues	23.7%	35.9%	40.4%	12.1%	26.9%	61.0%
Helped with hiring and dealing with employees	12.7%	24.7%	62.6%	7.3%	18.1%	74.5%
Helped with networking	28.7%	37.9%	33.4%	23.1%	31.2%	45.7%
Helped with using computers and technology	13.3%	26.5%	60.2%	12.1%	22.2%	65.7%
Helped with dealing with clients	16.7%	35.1%	48.2%	11.3%	30.4%	58.3%
Helped with providing psychological support	16.6%	31.0%	52.4%	13.1%	23.8%	63.1%

Notes: Sample includes treatment and control group participants who received any entrepreneurship training by wave 1 follow-up survey (six months). Evaluation of services was asked at W1.

Appendix Table 4—ITT Estimates, Compare to Table 4

	Intent-to-treat estimates		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.0464 (0.0166)	0.0517 (0.0153)	3,443
Business owner at W2 survey date	0.0216 (0.0179)	0.0208 (0.0172)	3,032
Business owner at W3 survey date	0.0095 (0.0197)	0.0025 (0.0194)	2,446
Monthly business sales at W1 survey date	−406 (282)	−369 (288)	3,210
Monthly business sales at W2 survey date	−186 (353)	−140 (353)	2,794
Monthly business sales at W3 survey date	−495 (539)	−620 (556)	2,323
Has any employees at W1 survey date	0.0131 (0.0092)	0.0140 (0.0095)	3,438
Has any employees at W2 survey date	0.0039 (0.0107)	0.0020 (0.0110)	3,023
Has any employees at W3 survey date	−0.0172 (0.0123)	−0.0209 (0.0128)	2,436
log household income at W1	−0.0239 (0.0319)	−0.0088 (0.0251)	3,223
log household income at W2	0.0353 (0.0357)	0.0195 (0.0293)	2,797
log household income at W3	0.0541 (0.0415)	0.0217 (0.0353)	2,270

Notes: The ITT model regresses the listed outcome on treatment. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 5—Impacts of Entrepreneurship Training on Business Ownership, Entry and Exit

Dependent variable	Treatment-control (IV estimates)		
	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.1252	0.1337	3,443
	(0.0446)	(0.0395)	
Business owner at W2 survey date	0.0742	0.0691	3,032
	(0.0616)	(0.0570)	
Business owner at W3 survey date	0.0406	0.0105	2,446
	(0.0844)	(0.0810)	
Started business by W1 (no business at application date)	0.1678	0.1595	2,690
	(0.0468)	(0.0446)	
Started business by W2 (no business at application date)	0.1017	0.0796	2,349
	(0.0663)	(0.0634)	
Started business by W3 (no business at application date)	0.0321	−0.0094	1,886
	(0.0893)	(0.0887)	
Exited business by W1 (had business at application date)	−0.0391	−0.0743	663
	(0.0854)	(0.0886)	
Exited business by W2 (had business at application date)	−0.0426	−0.0434	605
	(0.1284)	(0.1329)	
Exited business by W3 (had business at application date)	−0.1441	−0.1084	498
	(0.1971)	(0.2057)	

Notes: The first stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program site dummies, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 6—Impact of Entrepreneurship Training on Employed, Additional Business Outcomes, and Work Satisfaction

	Treatment-control (IV estimates)		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Employed (business owner or wage/salary) at W1 survey date	0.0681 (0.0385)	0.0698 (0.0360)	3,444
Employed (business owner or wage/salary) at W2 survey date	0.0707 (0.0464)	0.0643 (0.0435)	3,034
Employed (business owner or wage/salary) at W3 survey date	−0.0682 (0.0703)	−0.0785 (0.0671)	2,445
Number of employees at W1 survey date	0.2068 (0.2907)	0.0946 (0.2683)	3,438
Number of employees at W2 survey date	0.1173 (0.3134)	−0.1149 (0.2597)	3,023
Number of employees at W3 survey date	−1.0727 (0.7061)	−1.4223 (0.7462)	2,436
Monthly profits (sales minus reported expenses) at W1 survey date (000s)	−1.2459 (0.4429)	−1.1764 (0.4323)	3,146
Monthly profits (sales minus reported expenses) at W2 survey date (000s)	−0.1000 (0.6608)	0.1406 (0.5819)	2,736
Monthly profits (sales minus reported expenses) at W3 survey date (000s)	−1.2445 (1.3906)	−1.5505 (1.4154)	2,281
Business outcome index at W1	−0.0605 (0.0631)	−0.0518 (0.0611)	3,146
Business outcome index at W2	0.0129 (0.0889)	0.0174 (0.0849)	2,736
Business outcome index at W3	−0.2075 (0.1181)	−0.2377 (0.1212)	2,278
Total business income from all businesses owned from RA to W1 (000s)	−1.2562 (0.8526)	−0.7011 (0.8107)	3,229
Total business income from all businesses owned from W1 to W2 (000s)	−0.2474 (1.5748)	−0.3391 (1.5247)	2,754
Total business income from all businesses owned from W2 to W3 (000s)	−6.6298 (13.8269)	−10.0203 (14.0583)	2,222
Annual business income from current owned business at W1 (000s)	−1.1087 (0.9983)	−0.4360 (0.9536)	3,272
Annual business income from current owned business at W2 (000s)	−0.5841 (1.5026)	−0.8564 (1.4704)	2,830
Annual business income from current owned business at W3 (000s)	−1.9026 (4.5183)	−3.0512 (4.6203)	2,361
Work satisfaction: "very satisfied" at W1 survey date	−0.0007 (0.0455)	0.0153 (0.0450)	3,409
Work satisfaction: "very satisfied" at W2 survey date	0.0154 (0.0625)	0.0302 (0.0620)	2,992
Work satisfaction: "very satisfied" at W3 survey date	0.0430 (0.0998)	0.0511 (0.1031)	1,924

Notes: The first stage in the IV (LATE) model regresses receipt of entrepreneurship training on treatment. The second stage regresses the listed outcome on predicted receipt of entrepreneurship training. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance. The business outcome index is an equally weighted average of *z*-scores from sales, any employees, number of employees, and profits. *Z*-scores are calculated by subtracting the control group mean and dividing by the control group standard deviation.

Appendix Table 7—Distribution of Businesses by Home Based and Structure at Wave 3 for Treatment Group, Control Group, and US Total

	Treatment group			Control group			US firms	
	All individuals	All businesses	New businesses	All individuals	All businesses	New businesses	SBO (2007)	SBO (1997)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Business structure								
No business	61.1%	NA	NA	62.1%	NA	NA		NA
Sole proprietorship	22.5%	57.8%	56.7%	22.5%	59.4%	58.6%		72.6%
Partnership	1.6%	4.1%	4.8%	2.0%	5.3%	5.8%		5.9%
Corporation	12.0%	31.0%	31.3%	11.0%	29.0%	28.4%		21.0%
Other form	2.8%	7.1%	7.3%	2.4%	6.4%	7.3%		0.5%
Home based business								
No business	61.1%	NA	NA	62.1%	NA	NA	NA	
Not home based	9.0%	23.1%	24.7%	8.8%	23.2%	21.9%	38.0%	
Home based	29.9%	76.9%	75.3%	29.1%	76.8%	78.1%	62.0%	
Sample size	1,263	491	316	1,154	438	275		

Notes: United States totals in column 7 and column 8 are from the *Survey of Business Owners* 2007 and 1997, US Census Bureau, respectively. These data include all nonfarm businesses with sales of at least $1,000. New businesses are individuals who did not own a business at the time of application to the program.

Appendix Table 8—Treatment-Control Differences in Missing Values for Sales, Household Income, and Business Earnings

					Treatment-control	
Dependent variable	Treatment (1)	Obs. (2)	Control (3)	Obs. (4)	No covars (5)	Covariates (6)
Missing monthly business sales at W1 survey date	0.0722	1,758	0.0662	1,691	0.0060 (0.0086)	0.0068 (0.0089)
Missing monthly business sales at W2 survey date	0.0742	1,563	0.0868	1,475	−0.0126 (0.0099)	−0.0146 (0.0104)
Missing monthly business sales at W3 survey date	0.0487	1,274	0.0553	1,176	−0.0066 (0.0090)	−0.0110 (0.0093)
Missing household income at W1	0.0626	1,758	0.0686	1,691	−0.0060 (0.0084)	−0.0015 (0.0087)
Missing household income at W2	0.0800	1,563	0.0786	1,475	0.0013 (0.0098)	0.0051 (0.0101)
Missing household income at W3	0.0754	1,274	0.0714	1,176	0.0039 (0.0105)	−0.0002 (0.0110)
Missing total business income from all businesses owned from RA to W1	0.0660	1,758	0.0615	1,691	0.0045 (0.0083)	0.0024 (0.0086)
Missing total business income from all businesses owned from W1 to W2	0.0921	1,563	0.0949	1,475	−0.0028 (0.0106)	−0.0059 (0.0109)
Missing total business income from all businesses owned from W2 to W3	0.0871	1,274	0.0995	1,176	−0.0124 (0.0118)	−0.0111 (0.0121)
Missing annual business income from current owned business at W1	0.0529	1,758	0.0497	1,691	0.0032 (0.0075)	0.0005 (0.0078)
Missing annual business income from current owned business at W2	0.0691	1,563	0.0678	1,475	0.0013 (0.0092)	0.0002 (0.0095)
Missing annual business income from current owned business at W3	0.0369	1,274	0.0357	1,176	0.0012 (0.0076)	0.0004 (0.0080)

Notes: The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Treatment-control differences with covariates are estimated from a linear probability model that controls for program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance. The wave 3 sample for work satisfaction is restricted to include only the employed.

Appendix Table 9—Nonexperimental Correlations between Entrepreneurship Training and Outcomes for Control Group

	Nonexperimental estimates		
Dependent variable	No covars (1)	Covariates (2)	Obs. (3)
Business owner at W1 survey date	0.1965 (0.0234)	0.1533 (0.0225)	1,685
Business owner at W2 survey date	0.2579 (0.0247)	0.2212 (0.0251)	1,462
Business owner at W3 survey date	0.2277 (0.0277)	0.2098 (0.0297)	1,162
Monthly business sales at W1 survey date	836 (476)	561 (463)	1,575
Monthly business sales at W2 survey date	1,248 (478)	1,285 (533)	1,337
Monthly business sales at W3 survey date	2,058 (751)	2,035 (867)	1,101
Has any employees at W1 survey date	0.0354 (0.0130)	0.0304 (0.0132)	1,685
Has any employees at W2 survey date	0.0505 (0.0149)	0.0442 (0.0158)	1,457
Has any employees at W3 survey date	0.0678 (0.0176)	0.0641 (0.0186)	1,158
log household income at W1	0.1062 (0.0466)	0.0012 (0.0388)	1,571
log household income at W2	0.0396 (0.0522)	−0.0447 (0.0442)	1,348
log household income at W3	0.0833 (0.0653)	−0.0285 (0.0639)	1,082

Notes: In all regressions, the listed outcome is regressed on receipt of entrepreneurship training. The sample includes only observations for the control group. The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance.

Appendix Table 10—Separate Entrepreneurship Training Impact Regressions for Nonbusiness Owners and Business Owners at Baseline

Dependent variable	Main treatment (1)	Business owners × treatment (2)	Nonbusiness owners (3)	Business owners (4)
Business owner at W1 survey date	0.1552	−0.1048	0.1662	0.0725
	(0.0439)	(0.0876)	(0.0464)	(0.0865)
Business owner at W2 survey date	0.0707	−0.0914	0.0646	0.0252
	(0.0494)	(0.0975)	(0.0524)	(0.0968)
Business owner at W3 survey date	−0.0200	0.1217	−0.0060	0.0569
	(0.0555)	(0.1120)	(0.0588)	(0.1151)
Monthly business sales at W1 survey date (000s)	−0.9761	−0.9956	−0.4899	−3.3642
	(0.9818)	(2.5736)	(0.9754)	(3.0861)
Monthly business sales at W2 survey date (000s)	−0.4010	−0.0891	−0.2023	−0.4412
	(1.2896)	(2.6219)	(1.3195)	(2.9983)
Monthly business sales at W3 survey date (000s)	−0.9186	1.9006	−0.3707	−0.9437
	(1.5182)	(4.8169)	(1.5470)	(4.1919)
Has any employees at W1 survey date	0.0326	0.0039	0.0357	0.0891
	(0.0283)	(0.0891)	(0.0292)	(0.1110)
Has any employees at W2 survey date	0.0100	0.0082	0.0034	0.0693
	(0.0324)	(0.0949)	(0.0336)	(0.1173)
Has any employees at W3 survey date	−0.0143	−0.1008	−0.0231	−0.0529
	(0.0348)	(0.0927)	(0.0364)	(0.1090)
log household income at W1	−0.1793	0.4107	−0.2015	0.3470
	(0.0862)	(0.2033)	(0.0922)	(0.2001)
log household income at W2	−0.0308	0.3884	−0.0335	0.4678
	(0.0948)	(0.2058)	(0.1004)	(0.2253)
log household income at W3	0.0467	−0.0379	0.0590	0.0930
	(0.1123)	(0.2165)	(0.1191)	(0.2345)
W1 sample size	3,359	3,359	2,692	667
W2 sample size	2,960	2,960	2,353	607
W3 sample size	2,387	2,387	1,888	499

Notes: The wave 1, wave 2, and wave 3 surveys are conducted at 6, 18, and 60 months after time of application. Covariates include program sites, female, race, immigrant, age, married, children, education level, household income, self-employed at application, health problems, worked in family business, bad credit history, unemployment compensation, employer provided health insurance, autonomy, and risk tolerance. Each row and columns 1–2, 3, and 4 represent a separate regression.

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